

A gular pouch is mentioned by Berthold and Dr. Hallowell. Like that of *N. auratus*: the gular pouch is evidently possessed by the male *Norops* only.

The hind limbs are much shorter than in the preceding. The fourth toe is much longer than the third. The under surface of the toes is covered with transverse scales, which, however, are much narrower than the same scales or plates in *auratus*, as there is no appearance of dilatation in any of the toes.

The fore limbs, when stretched forward, reach to the extremity of the snout, the hind limbs only to the ear-opening.

The tail is thrice the length of the body.

Colours as stated above,—well described by Dr. Hallowell.

Localities given by different writers:—Surinam (Berthold), New Granada (Hallowell), New Granada? (Cope).

A specimen collected by Mr. Bates at Santarem, Amazons.

As both Dr. Hallowell and Dr. Peters agree in giving Mexico as the habitat of the *N. auratus*, the latter speaking of six specimens collected by Dr. Hille at Huanisco, it is not improbable that *N. auratus* is a truly Mexican species, while *12-striatus* is its South-American representative.

## XXVII.—On the Manner of Growth of Hyalonema.

By Dr. J. E. GRAY, F.R.S., V.P.Z.S., &c.

THE writers of anonymous papers in two scientific journals state that I have adopted Prof. Lovén's opinion that the *Hyalonema* grows rooted in the mud. I thought that by my paper I had sufficiently shown the difference between Prof. Lovén's and my theory. From the examination of the direction of the polypes and the form of the sponge of the specimens which had come into my possession, and the study of Dr. Max Schultze's description of the sponge of one of his specimens, I was convinced that the sponge to which the *Hyalonema* was attached could not be attached to any marine body by what Prof. Brandt, Prof. Max Schultze, and I have called its base, and that it must have lived with the so-called base upwards; and I believed that it did live free, with the free ends of the siliceous filaments sunk in the sand or mud.

Prof. Lovén, on the other hand, believes that all the specimens we have in museums are imperfect, and have been torn by force from a part of the specimen which is furnished with an expanded root and attached to some marine bodies.

Dr. William Carpenter does not appear to have a very clear idea of Dr. Lovén's paper; for in his very interesting "Preliminary Report of Dredging-Operations in the Seas north of

of the British Islands" (Proc. Roy. Soc. xvii.), he seems to have made the same mistake: thus at page 176 he states, "As it thus appears that these siliceous sponges, when growing on the surface of the mud, send root-fibres (so to speak) far and wide into its substance, the idea previously suggested by Prof. Lovén, that the elongated flint-rope of *Hyalonema Sieboldii* is in reality the mud-imbedded stem, supporting the sponge with which it is connected, instead of being implanted in the sponge and supported by it (which is the commonly received opinion), seems the more likely."

Prof. Lovén has made this distinction himself very plainly in a very amusing and instructive letter, which he most kindly sent to me immediately on the publication of my paper in the 'Annals':—

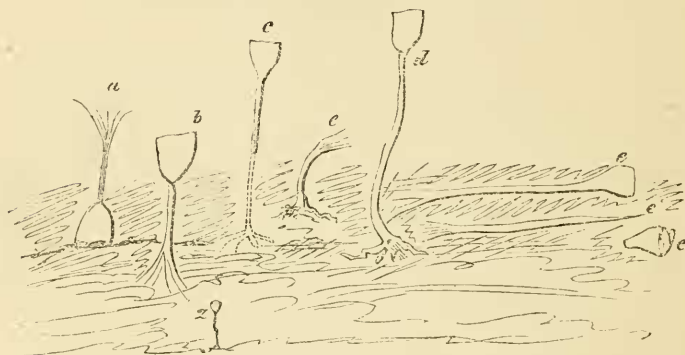
"I have just read your paper in the Ann. & Mag. 4th ser. No. 10; and, as I am not very fond of differing from you, I am glad to find that you now turn the old *Hyalonema* upside down—that is, place it with 'the sponge' upwards. This view, I see, is confirmed by Prof. Perceval Wright, who says, just as I maintained, that the 'siliceous axis' is the stem of the sponge; and also by Prof. Wyville Thomson, who found the *Hyalonema* growing upside down, which he might have more than 'suspected' from my paper. So far we all agree. As to what is now the lower end (formerly the upper) of the stem (coil), you are inclined to believe it to be 'sunk in the mud.' I cannot conceal that this mode of growing would be very unnatural indeed, unless you at the same time suppose the basal end to be provided with roots, in which case you have my *Hyalonema* complete.

"In order to settle the whole question, the best measure would be this: let orders be given to all the ships of your navy stationed in the Japan seas to dredge on the fishing-grounds off Inosima, and not to give up working till they have got entire specimens of the *Hyalonema*, with roots and all (every one to be preserved in strong spirits), and in a number sufficient for the glory of the British Museum\*. Among the contents of the dredge brought from the bottom, and of which not a particle is to be thrown away, there will be found, besides those entire specimens, in some of which the stem will have an extraordinary length, other specimens mutilated by the dredge or by fishermen's nets, some being the stumps of the basal parts with the roots, others the upper parts of the stem with the head or without the same, it having been

\* Allow me to suggest that perhaps one more specimen ought to be taken.

carried away by the dredge or by the nets, or otherwise; and on entire specimens, as well as on fragmentary ones, there will often be found the parasitic *Palythoa* investing them, and in some cases, where the head has been torn off, even creeping over the tip of the upper end of the stem and overgrowing it, as in the specimen you sent me.

"You see I venture to prophesy; and although it is said that 'no one is a prophet in his own country,' I may perhaps turn out to be one in the depths of the Japan sea. And when you have placed before you the superb specimens so procured, and the old ones too, you will have the history of the *Hyalonema*, as follows:—



"*a*, being the old notion.

"*b*, your supposition.

"*c*, my interpretation.

"*d*, what I expect from the dredge.

"*e*, disjecta membra.

"*z*, *Hyalonema boreale*, the innocent cause of this controversy."

As a note to the observation just quoted, Dr. Carpenter observes, as if he considered it a contradiction, that "Dr. J. E. Gray, whilst still maintaining that the flint-rope is a zoophytic product, and that the sponge with which it is connected is parasitic, has also come to the conclusion that the brush-like termination serves as the root implanted in mud, above which the sponge is borne."

It appears to me that the fact of Flint-Sponges and the zoophytic *Hyalonema* both having spicules sunk in the sand and serving as roots, may be an analogy as well as an affinity, considering that they both have to serve the same purpose of supporting the animal on a soft and yielding base, and that if the spicules were formed of calcareous matter they might be

acted on by the sea-water and the chemical constituents of the mud. The discovery of this use of the spicules of *Hyalonema* induced me to believe it might be the use of the long spicules of *Euplectella* and *Semperella*; and more recently Mr. Carter has shown that a *Tethya* is supported by similar elongated flinty fibres.

I must own that I am not convinced; and I do not think that I should be true to science and scientific truth if I did conform to any views which do not satisfy my doubts, or I should be most ready to give up my opinion if I were so, standing as I now do almost alone in my view of the question.

It does appear to me remarkable that we should have zoologists and physiologists of established reputation giving so decided an opinion on the subject, when they do not consider it necessary to reply to the reason that has been assigned why the spicules of *Hyalonema* are not sponge-spicules. It is true that *Hyalonema* and Sponges have siliceous spicules; but it is also shown that they occur in zoophytes, and that silica forms a large part of the constituents of stony corals. The siliceous spicules of sponges and *Hyalonema* have a central canal, which Mr. Carter has lately shown is not found in the calcareous spicules of sponges or zoophytes. The spicules of *Hyalonema* are formed, like the axis of zoophytes, of concentric layers; but no microscopist or physiologist has attempted to show me a siliceous spicule of a sponge that was formed of concentric coats, nor have they responded to my challenge to show me any spicule of a sponge that has the mode of growth or the external microscopical characters of the spicule of *Hyalonema*, which as a spicule is *sui generis*, and is more like the axis of a zoophyte than anything else. And why might not a zoophyte have a bundle of axes as well as one? They all harp on the one string that the spicules of *Hyalonema* are siliceous, and so are the spicules of most sponges, of all true sponges (for I think the calcareous animal bodies that have been called calcareous sponges belong to quite a different class), and therefore *Hyalonema* must be a sponge—I must say, a very lame conclusion when we consider how the siliceous spicules of *Hyalonema* differ in structure and mode of growth from the spicules of sponges.

The zoologists and physiologists have not shown me any sponge in which every spicule is surrounded by a regular coat of sarcode. They say that this sarcode is full of siliceous spicules of another form; but why, if the sarcode existed and formed one siliceous spicule, should it not form others of the same or other forms?

It has been objected that the *Palythoa* is so like the *Palythoa* that does not secrete siliceous spicules, that it must be a para-

site; but they forget that the animal of a Madrepora is very nearly allied to *Palythoa*—in fact only a *Palythoa* living in very crowded colonies and having a strong coral to protect it instead of a cartilaginous coat more or less strengthened with sand or spicules; and if the *Palythoa* of a Madrepora secretes 22 per cent. of silica in the same state of chemical combination as it is in the spicules of *Hyalonema*, why may not an allied species secrete silica that takes the form of spicules? The question is, I own, a very difficult one: but it is not to be solved by the *ipse dixit* of this or that Professor; it is one that requires careful study.

Unfortunately, some men of great reputation have, without sufficient examination and consideration, committed themselves to a theory, and they do not like to reconsider the question; but the time will come when it will be reconsidered; and if I am proved to be wrong, I shall have great pleasure in adopting their views and freely admit my mistake.

XXVIII.—*On the Habitat of the Regadera (Watering-pot) or Venus's Flower-basket (Euplectella aspergillum, Owen)*. By THOMAS J. MOORE, Free Public Museum, Liverpool.

DEAR DR. GRAY,

A few days since I received a note from Mr. S. R. Graves, M.P. for Liverpool, requesting me to call at his office to see some specimens which he thought would interest me. I went immediately, and Mr. Graves showed me two fair specimens of *Euplectella* which, with some others in still better condition, were brought to him by Capt. Robert Morgan, of the ship 'Robin Hood,' which vessel had just arrived in Liverpool from the Philippine Islands.

I fear I somewhat disappointed Mr. Graves when I told him we had already finer specimens in the Museum, from the first lot sold in England. Presently, however, Mr. Graves put in my hand an exceedingly clear and neatly written document by Capt. Morgan, detailing the place and mode of capture of these specimens, and illustrated by a rough sketch. This at once riveted my attention, as I could not call to mind any statement so definite and precise in any of the numerous papers published since the influx of these beautiful objects. I asked Mr. Graves's permission to publish the communication, which permission he kindly gave me, and promised that he would ask Capt. Morgan to call upon me; and I have this day had the pleasure of seeing him.

Capt. Morgan tells me that, after a tedious voyage among the Philippine Islands, he put into Cebú, to ship some sugar, and that he derived much of his information from a friend